Interactive comment on "A new balance formula to estimate new particle formation rate: reevaluating the effect of coagulation scavenging" by R. Cai et al.

Summary:

This work proposed a new formula to estimate particle formation rates during nucleation event. This method emphasizes the importance of coagulation scavenging effect among newly formed particles, especially in polluted urban area. The manuscript fits well to the scope of ACP and presents valuable methods/results. Thus I recommend it to be published after the following moderate/minor comments listed below have been adequately addressed.

Comments:

1. In the abstract (page 1, lines 28-29): The authors stated or cited previous study (Guo et al., 2014): "Continuous growth of nucleated particles also provides increasing aerosol surface area for heterogeneous physicochemical processes, which may contribute to haze formation." Is this conclusion/statement correct? Although many NPF events were regularly followed by an increasing of particle mass, this only indicates a high abundance of condensable vapors in the atmosphere. Actually, the newly formed particles can only grow up to 60~100 nm. The contribution by particles smaller than 100 nm to total aerosol surface area is not significant. Maybe the large number of grown particles can, through the coagulation process, grow into a larger size range, where they could contribute more efficiently to the particle mass concentration or particle extinction. But I think it is not proper, or at least it may result in certain misunderstanding, to directly connect the NPF (or continuous growth of nucleated particles) with haze formation. Please consider this issue.
2. In section 3 Experiment, did you consider the diffusion loss in the DMA and neutralizer, as recommended by Wiedensohler et al., (2012)?
3. Lines 138-141, why you chose 28000 cm^{-3} as a boundary, from visual expression or mathematic calculation? Could you explain it more? And in line 326, do you mean Fig. 2b?
4. Since you measured particle number size distributions down to ~1.5 nm, could you compare the $J_{1.5}$ between direct measurements (e.g., results in Fig.2b) and estimated using the experimental formula (Eq.11 in Kulmala et al., 2012). It would be interesting to check the uncertainty using the experimental formula to estimate $J_{1.5}$ in strong nucleation events.

5. Maybe it is better to move all the equations in section 4.2 to section 2, and only discuss the comparison results here.
6. I think it is unfair to include Eq. 8 in the discussion of section 4.2. Based on what I understand, it uses the different upper bound (6 nm), which is much lower than the bound (25 nm) in other equations. The better agreement of estimated $J_{1.5}$ is due to the "closer" $CoagS_m$ (d_m =4 nm) used in Eq.8. Could you recalculate $J_{1.5}$ using Eq. 8 with the same upper bound (25 nm) and different $CoagS_m$ (d_m = 2,3,4… nm), how the result correlates with that of Eq. 1.
7. Please also include the New Delhi case (Kulmala et al., 2005) in Fig. 6a. It also showed high *CS* values.
8. Did you observed any relatively clean nucleation events during this campaign? How about the comparison of different formula in clean case?
9. There are several grammar mistakes in the text, please carefully check.

References

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